Looking for Trouble

Looking for trouble: medical science and clinical practice in the historiography of modern medicine

The relationship between the pursuit of science and the practice of medicine has been a theme of abiding interest among medical historians. In consequence, we now possess a substantial body of research and writing which explores the ways in which science and medicine have interacted with one another from the early modern period to the present.[1] One recurring theme in this literature has been to highlight instances of tension and conflict between medical science and clinical practice, or between medical scientists and clinical practitioners. I should declare at the outset that I do not question the findings of such case studies: plainly scientists and clinicians did on many occasions come into conflict with one another. However, I would argue that historians of science and medicine have tended systematically to over-estimate the significance of such cases. Specifically, they have tended to suppose that tension between medical science and clinical practice has not just been an occasional occurrence, but is actually the normal or typical state of affairs, rooted in a divergence of culture and interest so profound that it is sometimes seen to constitute an essential difference between the two enterprises. Such suppositions have in turn informed historiographical practice, leading historians to concentrate on cases which confirm their assumptions regarding the normality of tension and conflict, and to regard those instances of cooperation that demonstrably did occur as exceptional and hence unrepresentative of the normal run of science-medicine relations.

My purpose in the present paper is to show how this historiographical predisposition came to dominate historians' accounts of science-medicine relations; to suggest some reasons why presumptions of conflict, rather than a more mutualistic understanding of science and medicine, might have become embedded in historians' perceptions; and briefly to point out how maintaining this point of view restricts opportunities for historians to engage in constructively critical ways with audiences beyond their own discipline.

Medicine, science and professionalisation

1. For overviews, see Warner 1995; Jordanova 1995.

In seeking to explain why historians have been so inclined to start from a presumption of tension between science and medicine, we might begin with the emergence, during the 1970s, of a sociologically-informed approach to the history of the professions. Previously, most medical historians, like many medical sociologists, had assumed that the relationship between science and medicine had on the whole been unproblematic; medicine and science had progressed hand-in-hand, as scientists applied their investigative talents to solving the problems faced by medicine, and doctors employed the findings of science in medical practice.[2] From the 1960s onwards, however, this comfortable view of the role of science in medicine began to be problematised by the growth of a distinctly critical movement within the sociology of medicine, concerned especially with issues of medical power and authority. Medical sociologists were increasingly inclined to see the development of medicine, not just as a story of technical progress, but also as the creation of a privileged and powerful profession. In a series of influential studies, sociologists including Elliot Freidson and Paul Starr portrayed the process of medical professionalisation from the late eighteenth century onwards as in effect a move to consolidate power over the laity and to exclude competitors from the medical marketplace. These sociologists rightly saw science as playing a crucial role in this process, arguing that the emerging medical profession adopted science not just as a source of sound natural knowledge and enhanced technical power over health and illness, but also as a form of esoteric expertise that placed medical judgment beyond challenge by the laity. For these authors, medicine's support for increasingly complex forms of science was part of a historic power grab, that transformed doctors from lowly tradesmen to lofty professionals who commanded respect from their patients and dominance over others working in the field of health care.[3]

This sociological perspective on the role of science in medical professionalisation found a sympathetic audience among medical historians, who were themselves becoming increasingly interested in explaining medicine's rise to social prominence and the emergence of its distinctly modern forms of social organisation. These historians were disposed to concur with the sociologists' view that doctors supported the growth of science as much for the authority it conferred as for any specifically technical power it

2. Fee 1989; Rosenberg 2007.

3. Freidson 1970; Jewson 1974, 1976; Starr 1982.

gave them over the body and its ailments.[4] Most notably, John Harley Warner's acute analyses of the rhetoric of science in late nineteenth-century American medicine made clear the multiple meanings that attached to "science" in medicine, and the different ways that physicians mobilised that term as a means of enhancing their own authority and status. In particular, Warner showed how, from the mid-nineteenth century onwards, doctors drew on the language of the new laboratory sciences to enhance their own "professional mystery", thereby reasserting their authority over their patients and marginalising those who advocated more democratic forms of medical practice.[5]

Meanwhile, historians of science too had begun to adopt a sociologically-informed interest in the professionalisation of science. A key focus of interest was the transformation of scientific research and teaching from a part-time activity conducted by amateurs (including medical practitioners) into a professional occupation in its own right, characterised by distinctive organisational forms including research schools and scientific disciplines.[6] Historians of the medical sciences were among the leading contributors to this historiographical project, producing groundbreaking studies of the establishment of experimental laboratory sciences during the late nineteenth and early twentieth century – most notably physiology, studied especially by Gerald L. Geison,[7] and biochemistry, as explored in the exemplary work of Robert Kohler.[8]

Historians of science were especially concerned to explain how sciences such as physiology and biochemistry won the support of the medical practitioners who controlled the medical schools within which they gradually secured an institutional base. To an extent, they concurred with medical historians in pointing to the kind of knowledge claims associated with laboratory science. The esoteric character of the laboratory

4. On science and medical professionalisation, see e.g. Peterson 1978; Shortt 1983; Morantz-Sanchez 1985; Fissell 1991; and, for an overview, Burnham 1998. On science and medical specialization, see Stevens 1966, 1998; Weisz 2006.

5. Warner 1991, 1992, 1995.

6. On research schools, see especially Morrell 1972; Geison 1972; Servos 1993. On scientific disciplines, key statements include Rosenberg 1979; Lenoir 1997.

7. Geison 1978; Geison (ed.) 1987. See also Coleman 1985; Fye 1987; and Coleman and Holmes (eds) 1988.

8. Kohler 1982. See also Weatherall and Kamminga 1996; van Helvoort 2002, 2003.

sciences, their commitment to the "experimental method", and their claim to fathom the fundamental phenomena of life itself, were all in keeping with a new ideology of technical expertise that at least some within the medical profession were keen to harness in their own pursuit of cultural authority and occupational monopoly.[9]

But laboratory scientists also had to tailor their activities to meet the expectations of their medical supporters, as Kohler showed in his detailed analysis of the establishment of biochemistry in British and American medical schools during the early twentieth century.[10] Biochemists won institutional support by performing various kinds of what Kohler called "service work" – providing pre-clinical training, routine diagnostic tests and other laboratory services – that clinicians found useful for their professional formation and in the business of diagnosis and patient care. Kohler sees this as implying a compromise: biochemists managed to create a space within which they were able to define their own disciplinary programmes of research; but they had to earn that space by devoting a significant part of their resources to serving a different agenda, defined by the medical profession. From the start, Kohler thus assumes a divergence between the biochemists' disciplinary aims and those of the clinicians they were compelled to serve.

Interprofessional tensions

The view of scientific professionalisation and discipline formation pioneered by Geison and Kohler did much to help displace older, teleological assumptions about science, which took for granted that scientific discovery would naturally lead to the forms and configurations of knowledge that exist in the present day. At the same time, it tended to reinforce a rather different, sociological form of teleology, explicit for instance in Freidson's and Starr's accounts of the rise of medicine, which assume that professionalisation is inherently oriented towards securing occupational dominance. The same view is evident in historians' assumption that the professionalisation of science tended naturally towards the establishment of disciplinary "independence", achievement of which serves as a mark of disciplinary "success" or "maturity".[11] Thus Kohler talks of

9. See, inter alia, Shortt 1983; Jacyna 1984; Jardine 1992; Warner 1992; Weatherall 1996.

10. Kohler 1982.

11. Golinksi 1998, pp. 66-78. Philosophers of science, too, have equated maturity with independence: for instance, see Kuhn 1977, pp. 237–38; and, for a critique, Barnes 2003.

how American biochemists "succeeded in establishing independent departments", whereas in Germany and Britain institutional factors resulted in "a more protracted, less successful process of discipline building".[12] Likewise Geison declares his 1987 volume on American physiology to be "dominated by one central theme: the emergence, maturation, and ascendance of physiology as an independent discipline in the United States".[13] The same equation of disciplinary independence with scientific maturity commonly recurs in other discussions of the formation of biomedical disciplines.[14]

This teleology colours the way that historians have tended to think about the relationship between medical science and clinical medicine. If the proper aim of scientific disciplines is independence, any activities that serve other disciplinary or professional agendas must represent a diversion from that aim. This is apparent, for instance, in Kohler's view of biochemical "service work" as a mark of the discipline's continuing dependence on clinical medicine. A similar perspective is evident in the assumption that the physiology departments in late nineteenth-century Oxford and Manchester were "failures" because they continued to devote much of their energy to training students for medical practice rather than building research schools comparable to that at Cambridge,[15] or that medical science in early twentieth-century Glasgow became "isolated from the forefront of scientific advance" in consequence of scientists' continuing commitment to a clinical service role.[16]

Much of what is now seen as canonical work in the history of science and medicine was thus informed by an inherently teleological understanding of

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12. Kohler 1982, pp. 6-7. Kohler later notes that, prior to 1940, many American biochemistry departments retained a "close connection to clinical medicine" that "may appear to contradict the idea ... that biological chemists liberated themselves from clinical medicine" at this time. He resolves this apparent contradiction by arguing that the relationship was no longer one of dependence, but rather "a more or less equal partnership" in which "clinicians as well as biochemists were obliged to adapt their disciplinary ideals". Kohler 1982, p. 215. Similarly equal partnerships are also seen to characterize new forms of "biomedicine" that emerged in the post-war years: Gaudillière 2002; Keating and Cambrosio 2003.

13. Geison 1987, p. 8.

14. For instance Morman 1984; Parascandola 1992; Prüll 1998.

15. Butler 1988; Romano 1997.

16. Smith and Nicolson 1989, quoting p. 210.

professionalisation as the pursuit of professional autonomy, authority and ultimately power. And this implied that, where the interests of the two professions overlapped, as for instance in the understanding and treatment of disease, conflict should be expected. Historians had little difficulty finding instances where that that was exactly what occurred. In a series of key articles that appeared from the mid 1970s onwards, historians of medical science analysed a number of late nineteenth- and early twentieth-century disputes between doctors and scientists over the proper place of science, and particularly the laboratory sciences, in medicine. They demonstrated that disputes tended to break out especially when scientists drew on their disciplinary knowledge and expertise to challenge doctors' accounts of disease and to assert their own claims to know best how to diagnose and treat it in the clinic.[17] Doctors, for their part, actively resisted such incursions into their own sphere of authority, and sought instead to ensure that new scientific knowledge and techniques should remain subordinate to clinical judgement. In a much-cited paper, for instance, Stephen Jacyna showed how surgeons in Glasgow's Western General Hospital adjusted their working practices to make use of new laboratory-based diagnostic tests conducted by their scientist colleagues, while ensuring that they nevertheless retained overall authority over clinical decision-making.[18]

Increasingly, historians came to see such inter-professional tensions as the norm. We can see this, for instance, in Keith Wailoo's study of the reception of laboratory evidence for the existence of morphologically abnormal red blood cells in certain cases of anaemia. In an otherwise insightful analysis, Wailoo simply asserted the unsubstantiated claim that clinicians' scepticism regarding the significance of that evidence was "nurtured by the era's pervasive tensions between the laboratory and the clinic"; only by "fully subordinating and standardizing laboratory technique to meet clinical interests" would clinical acceptance of the diagnostic status of sickle cells be achieved, he argued.[19] In effect, teleological assumptions about the supposed struggle for professional dominance had acquired explanatory status in their own right, without the need for further empirical demonstration.

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17. Hall 1976; Geison 1979; Maulitz 1979. Less well known, but in similar vein, is Parascandola 1982. Other studies showed how some doctors also drew on new forms of science to claim epistemic authority within medicine: e.g. Warner 1980.

18. Jacyna 1988.

19. Wailoo 1991, pp. 187, 200-201.

Against this tendency to suppose an inherent tension between the professional interests of science and medicine, we can set a growing body of research that takes a less agonistic view of professionalisation and discipline formation. More recent sociological analyses of professionalisation have moved on from considering how individual professions acquire authority and dominance, and have adopted a more systematic perspective on the negotiated alliances, inter-dependencies and jurisdictional boundaries that make possible a constantly shifting division of expert labour.[20] Some historians of science and medicine, too, have adopted such a perspective. Adele Clarke's 1998 study of the emergence and consolidation of the field of reproductive science is a case in point. Clarke is less interested than earlier historians of science and medicine in the pursuit of disciplinary independence or the defence of clinical dominance, and less inclined to privilege research and the work of scientific knowledge production over activities such as medical training or the provision of diagnostic services. That is not to deny that reproductive scientists' success in generating new knowledge was an important factor in determining precisely how and where their field came to be located within the larger patchwork of medical sciences and practices. But the epistemic work of knowledge production was only one element in the emergence and stabilisation of reproductive science, and cannot be seen in isolation from other processes including the production and exchange of research materials and the provision of other services to adjacent biomedical fields, among which clinical medicine was an important partner.[21]

From this point of view, teleological assumptions about the necessary divergence of medical scientific disciplines from the concerns of clinical practice plainly cannot be sustained. A similar perspective is increasingly evident in other research into the history of biomedical disciplines. In particular, an impressive body of work in the history of immunology has made clear the extent to which immunological knowledge and practice was shaped, not just by the interests of professional scientists, but also by problematics, perspectives and practices that developed within the sphere of clinical medicine.[22]

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20. Notably Abbot 1988. Burnham suggests that Abbot's revisionist perspective owed much to the empirical work of medical historians: Burnham 1998, pp. 156-157.

21. Clarke 1998. See also Sengoopta 2006, Krementsov 2008, for the prominence of clinicians in the development of endocrinology.

22. Löwy 1992; Anderson et al. 1994; Jackson 2007; Kroker et al. (eds) 2008.

Despite this, teleological assumptions about the nature of the relationship between biomedical science and medical practice continue to inform historians' implicit expectations, if not their explicitly programmatic pronouncements. For instance, in an excellent study of the development of radioimmunoassay in the years after the Second World War, Angela Creager has recently documented the "thick connections between the clinic and the laboratory" that underpinned that development. Specifically, she shows that "the technique emerged out of clinical research, and then moved into biological research as well as into medical diagnostics". Strikingly, Creager herself professed to find this "surprising".[23] That she should be surprised by such findings is a clear indication of how far even the most sophisticated historians of the biomedical sciences continue to assume that scientists are generally inclined to pursue their own independent research programmes and to reject initiatives and perspectives that derive from clinical medicine.

Cultural conflict

Closely allied to work on the professionalisation of science and medicine are a number of studies that trace tensions between the two professions to underlying differences in culture and values. These include accounts of the sometimes heated arguments that broke out over efforts to reform academic medicine in the US and Britain during the first half of the twentieth century. In the US, efforts to reform medical teaching were pursued with especial vigour by the Rockefeller Foundation under the banner of what they called "scientific medicine",[24] while in Britain the Medical Research Council (MRC) adopted a similar programme of reform in the name of "clinical science".[25] Both programmes set out to reorganise clinical medicine in ways that were explicitly modelled on new scientific disciplines such as physiology and biochemistry. This included replacing part-time clinical teachers with full-time academic professors of medicine and surgery; promoting the use of laboratories for clinical teaching and research; and making success in research rather than practice the main criterion for appointment to the new academic chairs. Unsurprisingly, academic scientists often

23. Creager 2008, pp. 201-202.

24. Brown 1979; Berliner 1985; Wheatley 1988. See also Cueto 1994; Gemelli et al. (eds) 1999; and Schneider (ed.) 2002 for Rockefeller efforts to export their programmes beyond the US.

25. Graham 1970; Fisher 1987; Austoker 1988; Austoker and Bryder (eds) 1989.

supported such reforms, while the private medical practitioners who still dominated clinical teaching at that time tended to resist them, rightly seeing them as a threat to their own institutional identities and interests.

Historians have tended to see the disputes surrounding the Rockefeller and MRC programmes as yet another instance of a more general clash of interests between laboratory scientists and clinicians. Persuaded perhaps by the rhetoric of "scientific medicine" and "clinical science", some historians have tended to equate the Rockefeller and MRC programmes with the promotion of science in general, and to suppose that clinicians' resistance to those programmes represented an essentially reactionary defence of unscientific or even anti-scientific values.[26] This interpretation sits oddly alongside the view that medical practitioners were keen to ally themselves with science as a source of cultural authority. Nonetheless, the supposition that the Rockefeller reformers represented a distinctly modernist configuration of scientific and bureaucratic values, while their opponents embodied a characteristically conservative clinical culture, remains pervasive.[27]

This view is reinforced by more detailed research into the professional and disciplinary cultures of laboratory science and clinical medicine. Notably, much of that research was informed by precisely the kinds of teleological suppositions about the pursuit of professional dominance and disciplinary independence that I discussed in the previous section. Thus early work on the culture of laboratory science sought among other things to elucidate the means by which scientists asserted their independence from medicine. These included privileging original contributions to research as the most important means of advancement within the profession; the growth of discipline-specific societies and journals through which the products of research could be subjected to intra-disciplinary peer evaluation and approval; the establishment of increasingly complex and arcane methodologies of knowledge production, in particular an emphasis on experimentation over more observational methods; and the creation of laboratories, equipped with sophisticated measuring instruments and other technologies of control, as sites both for the pursuit of experimental research and for the reproduction of disciplinary culture through training of new recruits. Such innovations effectively defined a new

26. Notably Brown 1979 and Berliner 1985.

27. For instance Bynum 1995; Lawrence 2005.

scientific culture distinct from that of medicine, and open only to those prepared to invest the time needed, first, to acquire the necessary knowledge and skills, and then to undertake the requisite research.[28]

Likewise, accounts of the culture of clinical medicine have in part been driven by interest in how clinicians sought to assert their own cultural and epistemic authority against incursions by laboratory scientists. Probably the most influential work in this respect is a series of studies by Christopher Lawrence that explore the cultural values espoused by the élite physicians and surgeons who taught in the great London teaching hospitals during the late nineteenth and early twentieth century. Lawrence argues that within the professional world of these medical "patricians", achievement was measured more in terms of the ability to attract high-paying and preferably upper-class private patients than by original contributions to research. This in turn sustained a value system that elevated individualism over disciplinary teamwork, clinical experience and diagnostic acumen over narrowly instrumental forms of technical skill, tradition over innovation, and gentlemanly accomplishments such as a good general education, elegant manners and the outward trappings of landed wealth over specialist expertise.[29] Such values were clearly at odds with those that historians have identified as central to the disciplinary culture of professional science. Lawrence himself is careful to make clear that the London clinicians were not necessarily hostile to scientific innovation per se. On the contrary, provided it was kept in its place, many saw it as an invaluable source of new knowledge and techniques. Their concern was simply to ensure that their own clinical authority was not usurped by a new breed of professional scientists.[30] Other historians have sometimes been less circumspect in their conclusions, and Lawrence's work is

28. Much of this work concentrated on physiology as the epitome of disciplinary independence. Thus Geison 1978; Geison (ed.) 1987; and Coleman and Holmes (eds) 1988 all cover several of the themes just mentioned, while Fye 1987 stresses the role of the "research ethic" in the institutionalisation of American physiology. On the role of distinct disciplinary projects and methodologies, see Coleman 1985; on societies and journals, see Brobeck et al. (eds) 1987, especially the chapters by Appel; on instruments, Borell 1987 and de Chaderevian 1993; and on the laboratory as embodying a culture of technical control, Todes 2002.

29. Lawrence 1985a, 1998, 2000. He finds a similarly "patrician" culture in interwar Edinburgh: Lawrence 2005, p. 330.

30. Especially Lawrence 1999.

widely cited as evidence of a more general "antagonism amongst some élite physicians to the new ethos of 'scientific' medicine".[31]

Of course, laboratory testing and other new technologies did in the event become deeply embedded in medical culture in the course of the first half of the twentieth century. But insofar as medical historians have sought to explain this transformation, they have tended to portray it, not as a process of evolution, in which an older medical culture developed into a new one, but rather as a process of displacement, in which the old medical culture was replaced, in whole or in part, by new forms of life that derived primarily from the laboratory. In some instances, historians have invoked wider cultural forces – notably the growth of organised health care, with its emphasis on the management of patients and doctors alike – to explain this displacement.[32] But whatever the wider explanatory strategy, such studies still tend to perpetuate an agonistic supposition that laboratory science and clinical medicine embodied divergent cultures and values, and that their eventual rapprochement was only achieved through the subordination of one culture by the other.

Implicit within such studies, then, is a tendency to reify particular cultures of laboratory science and clinical medicine. This is evident in the way that historians routinely talk of "the laboratory" and "the clinic", or of "bench" and "bedside", as if these were generally unproblematic – and unproblematically distinct – objects of observation and analysis. In fact, considerable care needs to be taken in talking about different places and cultures of scientific work. As a number of historians of science have pointed out, there is no historically fixed definition of what constitutes a laboratory.[33] Laboratories could be more or less continuous with other workspaces, including industrial workshops, kitchens and other domestic spaces – and one might equally well add hospital wards and clinics to this list. Moreover, laboratories could harbour precisely the same gentlemanly values that Lawrence discerns in the culture of élite medicine;[34] while conversely, clinical settings could serve equally well with

31. Morus 1999, p. 264.

32. Howell 1995; Sturdy and Cooter 1998.

33. E.g. Gooday 2008; Kohler 2002.

34. Schaffer 1998.

laboratories as disciplined sites of knowledge production.[35] Plainly we cannot presume any necessary distinction between the cultures of laboratory science and clinical medicine, let alone assume antagonism between them. Yet historians of medicine often continue to talk as if these were unproblematic categories.

Conflicting epistemologies

As Lawrence shows, a particularly important element in clinicians' defence of their professional authority was their articulation of a philosophy of medicine which insisted that while clinical practice might make use of science, it was not itself a science. Lawrence enumerates several ways in which advocates of this view argued that medicine differed from science. Thus clinical medicine was seen to be oriented, not towards the production of natural knowledge, but towards the humane work of curing and caring for patients. Consequently, clinical knowledge was rooted in practical experience of individual cases of illness, and was necessarily holistic, intuitive and inductive. Ultimately, claimed the clinicians, medicine was an art, not a science; as such, it involved a distinct epistemology and methodology from that which characterised science.[36] Other historians have since documented further instances of this kind of holistic and humanistic rhetoric, as well as exploring the relationship between that rhetoric and the way clinical practice was organised and conducted.[37]

In fact, arguments about the status of clinical knowledge have remained current to the present day, repeatedly resurfacing whenever questions about the proper aims and methods of medicine are mooted. Such questions became particularly pointed during the 1960s and 1970s, when doctors found themselves caught between conflicting critiques. On the one hand, an unlikely assortment of radicals and conservatives began arguing that medicine was becoming overly scientific, and that a heartless pursuit of scientific values was proving inimical to the preservation of patients' rights and dignity.[38] On the other

35. Wilde and Hirst 2009; Adams and Schlich 2006. For a less successful attempt to discipline clinical observation in general practice, Marks 2006.

36. Lawrence 1985a, 1998.

37. See the various studies collected in Lawrence and Weisz (eds) 1998. Also Cantor 1990, 2002, 2005.

38. Prominent among the conservative critiques was Illich 1975. More radical critiques were fuelled by the emergence of the civil rights and other social movements: Rothman 1991.

hand, medicine came under criticism from reformers who argued that medicine was not yet scientific enough, that too many of its procedures were unvalidated by statistical and other forms of scientific scrutiny, and that effective solutions to the ailments that beset society would only be secured through aggressive scientisation of the medical system.[39] In this context, the claim that medicine should be understood as an art, not a science, acquired renewed salience as a means for doctors to rebut criticisms from both sides.[40]

Doctors were not alone in formulating this claim. Philosophers as eminent as Stephen Toulmin rallied to medicine's cause, articulating sophisticated epistemological reasons why medicine was an art which might draw on but could never be reduced to science. Their arguments involved contrasting the forms of empirical reasoning on which medical practice is based with those they considered to be characteristic of science. Thus where science is the pursuit of universal truths, medicine revolves around knowledge of sick individuals, and as such requires an understanding of complex, concrete particularities, not abstract generalisations; where science is analytic, medicine is holistic and synthetic; where science proceeds through deductive reasoning, medicine relies upon inductive judgements; where science requires a critical distance from the object of inquiry, medicine involves a sympathetic and subjective identification with the patient and a hermeneutic understanding of their complaints.[41] Such rigorous philosophical arguments lent considerable credibility and gravitas to doctors' claims that medicine and science are fundamentally different kinds of enterprises, and that attempts to turn medicine into a science were therefore misguided.

This statement that medicine is not a science because it does not conform to scientific standards of epistemology is plainly an essentialist one: it turns on the presumption that science can be demarcated from non-science by noting the presence or absence of what are taken to be essential features of scientific observation and rationality. More recently, however, work in post-Kuhnian philosophy and sociology of scientific knowledge has made clear that such epistemological essentialism is unsustainable. Among other things, this work argues that scientific knowledge itself involves knowledge

39. Berg 1995; Marks 1997.

40. E.g. Daly 2005; Schlich 2007.

41. For instance Toulmin 1976; Engelhardt et al. 1979; Pellegrino and Thomasma 1981; and more recently, Gadamer 1996.

of concrete particulars, and is itself necessarily inductive, holistic and hermeneutic.[42] Consequently, it is simply not possible to demarcate clinical knowledge from scientific knowledge on epistemological grounds. Rather, any attempt to draw such a demarcation must itself be seen as a rhetorical move to delineate and defend a distinct sphere of jurisdiction or authority – an instance, in other words, of what sociologists of scientific knowledge call "boundary work".[43]

This is borne out by a number of empirical studies which make clear the ideological and instrumental character of claims about the nature of clinical knowledge. Talk of "holism", the "clinical art", and the irreducible character of medical experience and skill, all serve as malleable rhetorical resources, to be deployed in whatever way might best serve in to underwrite the status of clinical expertise.[44] Indeed, it might be noted that scientists too sometimes use the language of art to reinforce their own claims to expertise and authority.[45] Consequently, if we are to understand clinicians' use of such language, and their opposition of that language to what they identify as the dangers of science, we cannot adopt a realist reading. Rather, we must locate that usage in its specific social historical context, and analyse the particular purposes for which it was mobilised in that context.

Nonetheless, essentialist arguments about the supposed epistemological differences between science and medicine continue to be reproduced, not just by philosophers, but also by some working within social and cultural studies of medicine. Most prominently, in her celebrated work on medical narratives, Kathryn Hunter expressly states that the narrative, hermeneutic, case-based character of clinical knowledge means that medicine cannot be a science.[46] Others, including some historians, have suggested that the fact that new clinical technologies were often developed on the basis of observation and experience, rather than through laboratory experimentation,

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42. Kuhn 1970, pp. 189–90; Barnes 1982, pp. 45–53, 70–83; Forrester 1996; Nickles 2003; Creager et al. 2007.

43. See especially Gieryn 1983, 1999; Gordon 1988.

44. Sadler 1978; Anderson 1992; Rosenberg 1998; Löwy 2008.

45. Cambrosio and Keating 1988.

46. Hunter 1991, pp. xvii, xix-xxi, 18, 20, 28, 44-45.

should be seen as a further instance of clinicians' resistance to the epistemology of "scientific medicine".[47] In the absence of any evidence that clinicians actually saw new technologies in this light, such claims are pure speculation – albeit of a kind that evidently enjoys considerable credibility among historians of medicine.

Instances of cooperation

If historians of medicine are often inclined to suppose that the relations between laboratory science and clinical practice are typically marked by divergence, tension and sometimes open hostility, we can also point to a number of case studies that make clear that, in some instances at least, the clinical work of caring and the scientific work of producing systematic natural knowledge were intimately entwined with one another. Under the right circumstances, professional scientists and clinical practitioners could and did collaborate to produce medical knowledge that met both the standards of disciplinary science and the immediate needs of clinical practice.

For instance, Christopher Lawrence has shown how the field of cardiology was transformed during the early twentieth century through sharing of methods and ideas between clinicians and experimental physiologists.[48] Susan Leigh Star, meanwhile, has shown how cerebral localisation research in the years around 1900 developed through close collaboration between neurological surgeons and experimental physiologists.[49] Despite differences in the professional cultures they inhabited, physiologists and neurological surgeons were able to bring their activities into correspondence with one another, in ways that reduced the epistemic, practical and social uncertainties from which each field on its own was seen to suffer. More recently, Stephen Jacyna has shown that the development of aphasia research likewise depended upon both clinical observation and laboratory experimentation to identify those parts of the brain concerned with speech functions. Jacyna also makes clear that experimental knowledge of aphasia actually drew on and incorporated many of the narrative elements of clinical knowledge, and the kind of appreciation of the patient's subjective experiences, that Hunter, for one, sees as

47. For instance Hayter 1998.

48. Lawrence 1985b.

49. Star 1989.

setting clinical judgement irrevocably apart from scientific knowledge.[50] In a similar vein, Christopher Crenner has shown how new laboratory technologies served not to negate but rather to redefine medical humanism and medical ethics in the work of the Boston physician Richard Cabot.[51] And more recently still, my own research on the early twentieth-century Edinburgh medical school has shown how clinicians and laboratory scientists collaborated promiscuously in a broad spectrum of scientific investigations that ranged from elucidation of obscure clinical cases at one extreme to experimental research into animal physiology at the other. In this setting, at least, scientists and clinicians occupied a shared culture of scientific medicine that encompassed both clinic and laboratory as places of work, and that combined knowledge production with the diagnosis and treatment of patients in a single joint enterprise.[52]

Such studies make clear that while professional, cultural or epistemological tensions did on occasion occur between medical scientists and medical practitioners, they were by no means inevitable. It was perfectly possible to combine basic science and clinical medicine within common programmes of work, in which the production of new knowledge and the diagnosis and treatment of patients were intimately connected, and in which laboratory and clinic served as different but complementary sites of scientific work. Despite such clear counter-examples, however – and despite occasional objections from other historians of medicine[53] – the supposition that science-medicine relations were typically tense and even conflictual throughout the second half of the nineteenth century and the first half of the twentieth remains pervasive. Certainly, that is how the existing historiography has been read by younger scholars coming into the field, growing numbers of whom now perceive a need to counter what they see as the prevailing interpretative orientation of work in this area.[54]

Conclusion

50. Jacyna 2000, pp. 167–70, 185–89. On narratives in other spheres of scientific knowledge-making, see e.g. Myers 1990; Harré 1991.

51. Crenner 2005.

52. Sturdy 2007.

53. Palladino 1999; Hull 2007.

54. For instance Hammerborg 2010; Wall 2010.

What sustains historians' tendency to dwell on tensions between science and medicine? I have pointed to a number of trends within the historiography of science and medicine that embody this tendency. As we have seen, the development of these trends from the 1970s onwards coincided with debates within medicine itself, and especially in the philosophy of medicine, about the aims and organisation of medicine and its proper relationship with science. Many saw the history of medicine as an opportunity to interrogate that relationship, and in particular to refute overly cosy and triumphalist assumptions about the inevitable and beneficial progress of scientific medicine. There was thus an obvious attraction in uncovering instances of disagreement, and historians' accounts of such instances have since become part of the canon of our discipline.

At the same time, however, historians had an incentive to buy into the kind of oppositional rhetoric of science and medicine that became so prominent during the 1970s, and that remains salient to the present day. Many historians of medicine – particularly those whose professional commitments include the training of medical students – see their work not just as a contribution to historical scholarship, but as a means of helping doctors to reflect on their own place in society. In particular, ancillary teaching in the history of medicine is often justified as providing a humanistic counter-balance to the predominantly scientific training on offer in the rest of the medical curriculum.[55] Consequently, medical historians have a vested interest in perpetuating the same distinction between scientific and non-scientific values as commonly features in doctors' own rhetoric about the nature of their craft. This is perhaps most clearly expressed in the emerging discipline of medical humanities, which is often explicitly framed as a means of reasserting the centrality of medical art and mitigating the supposedly dehumanising tendencies of scientific medicine.[56] But it is also evident in medical historians' inclination to assume an inherent tension between clinical humanism and the expansion of medical science.

Reasserting and reinforcing doctors' own rhetoric in this way is to do a disservice to the history of medicine, and perhaps to medicine itself. This is not just because a polarising historiography misrepresents the way that medicine and science have

55. For instance Jackson 2002.

56. E.g. Hunter 1991, pp. xix, xxi.

interacted with one another. It also vitiates historians' ability to comment constructively on what medicine is and might be. By reproducing claims that medicine is necessarily something other than science, medical historians embroil themselves in intra- and interprofessional debates over medical authority that they might more appropriately seek to analyse and explain. Historians are equipped to do more than simply shore up the ideological distinction between medical science and the medical art. In recognising the rhetorical nature of that distinction, historians possess the means to challenge the presumptions on which it is based, and to effect a far more penetrating analysis of the relationship between language and practice, knowledge and social order in the field of medicine as a whole.

With this as their starting point, historians are in a strong position to initiate a more profound discussion of how medicine should be organized and delivered. The choices are not simply between science and art, laboratory and clinic; they are manifold, embedded in the fine grain of medicine's daily practices, and at once epistemological and ethical. History, once freed from polarising dichotomies, provides the material with which to reflect on the sheer diversity of medicine's past, and the perspective from which to imagine how the present might be reinvented. By reverting to an essentialist reading of the relationship between science and medicine, historians unwittingly take a step backwards from the possibility of such reflection.

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